

and repulsions of floating bodies : these attractions are found to vary ultimately in the inverse ratio of the squares of the distances ; and they appear to be the same as are found to cause an apparent cohesion between any moistened surfaces nearly in contact : the magnitude of this cohesion, as measured, in a particular case by Morveau, being found to agree with the calculation of the effect of capillary action.

The attraction of a drop of a fluid towards the line of contact of two plates of glass, which was found by Hawkesbee to vary nearly in the inverse ratio of the square of the distance of the plates, was supposed by Newton to indicate an immediate cohesive force, varying in the simple inverse ratio. But Dr. Young has shown that the fundamental law of the equable tension of the surface is sufficient to explain this phenomenon, and to remove the apparent irregularity in the laws of cohesive forces.

The equable tension of the surface is shown to be a consequence which may be mathematically deduced from the existence of a stable equilibrium between the forces of repulsion and of cohesion, which is a necessary condition of liquidity, as the repulsive force always varies more rapidly than the cohesive force. The mutual attractions of solids and fluids are then considered ; and Dr. Young agrees with Clairaut, although upon different grounds, in affirming that a fluid will be elevated when in contact with any solid of more than half its attractive density. The tension of the common surfaces of a solid and a fluid, or of two continuous fluids, is supposed to be proportional to the difference of the attractive densities ; and this supposition is confirmed by some observations, with which the paper is concluded, on the phenomena of oily substances floating on water.

*Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. Read January 24, 1805. [Phil. Trans. 1805, p. 88.]*

This paper may be considered as a continuation of Mr. Knight's former communications respecting the motion of the sap in trees. Du Hamel, and other subsequent naturalists, have shown that trees contain two kinds of sap ; and the chief purpose of Mr. Knight's paper is to prove that one of them (called by Du Hamel *suc propre*, and by Mr. Knight the true sap,) is generated in the leaf ; and that this fluid, in an inspissated state, or some concrete substance deposited by it, exists during the winter in the albumum, from which substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring. To the above-mentioned deposition, Mr. Knight attributes the well-known superiority of winter-felled wood, which superiority has generally been supposed owing merely to the absence of the sap at that season.

Du Hamel has remarked, that trees perspire more when the leaves

are full grown, and when the annual shoots have ceased to elongate, than at any earlier period. This energy in the powers of vegetation must certainly, Mr. Knight thinks, be employed in some very important operation. He has observed that the produce of his meadows has been greatly increased when the herbage of the preceding year had been left till the end of autumn, on ground that had been mowed early in the summer; from which he has been led to imagine, that leaves are employed during the latter part of the summer in the preparation of matter calculated to afford food to the buds and blossoms of the succeeding spring.

In order to determine whether the foregoing opinions were well founded or not, Mr. Knight made the following experiments.

Having made incisions in the trunks of sycamore and birch-trees (some of these incisions being close to the ground, others at the elevation of seven feet), he found that the sap obtained from the sycamore close to the ground, was of the specific gravity of 1.004, while that obtained at the height of seven feet had a specific gravity of 1.008. The sap of the birch was somewhat lighter; but the increase of specific gravity, at different elevations, was comparatively the same. The sap of both these trees, when extracted near the ground, was almost void of taste; but when obtained at a greater height, it was sensibly sweet. In one instance it was extracted from the sycamore-tree at the height of twelve feet; it was then very sweet, and its specific gravity was 1.012.

Mr. Knight then made an experiment to compare the sap obtained from a recent incision with that obtained from an old one. He found that the sap from an old incision was reduced in specific gravity to 1.002, while that from the recent incision continued at 1.004, as before. These incisions were made in a sycamore-tree, and were close to the ground.

Some observations then follow on the variation in the specific gravity of the alburnum at different seasons. After taking every precaution to avoid error, the author found the specific gravity of winter-felled-oak to be 0.679, and that of summer-felled oak to be 0.609, after they had both been immersed five minutes in water. This difference appearing to Mr. Knight very considerable, he repeated the experiment several times, but found no reason to suspect any error in it; and upon measuring pieces of both kinds of wood, which were equal in weight, it appeared that the winter-felled pieces were much less than the others. The more recently formed layers of winter-felled wood had a specific gravity of 0.583; that of the summer-felled wood was only 0.533. In another experiment the former was 0.588, the latter 0.534.

On pouring boiling water on equal quantities of summer- and of winter-felled wood, it appeared that the latter communicated a much deeper colour to the water than the former; it also raised the specific gravity of the water to 1.002; the specific gravity of the other infusion was 1.001.

Mr. Knight thought he had reason to believe that the matter de-

posited in the alburnum sometimes remains unemployed during several successive years; he therefore cut off, in the winter, all the branches of a large and very old pear-tree, at a small distance from the trunk, and pared off, at the same time, all the lifeless external bark. No marks of vegetation appeared till the beginning of July following, when numerous buds and leaves, of large size, appeared; and in autumn every part was covered with very vigorous shoots. The number of leaves appeared to Mr. Knight to exceed very much the whole of those the tree had borne in the three preceding years.

Mr. Knight says that he has repeated, with success, the experiments of Bonnet and Du Hamel, and that he is in possession of many other facts which, like those experiments, tend to prove that seedling trees depend, at first, entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and often killed, by being put to vegetate in rich mould. He thinks there is very decisive evidence that bulbous and tuberous-rooted plants contain within themselves the matter which subsequently composes their leaves; also that it appears extremely probable, that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many plants precede their leaves.

Mr. Knight also thinks the existence of a vegetable circulation, though denied by many eminent naturalists, must be admitted. He supposes that when a seed is placed in a proper situation for vegetation, water is absorbed by the cotyledons, and a young radicle is emitted. This increases in length, by the addition of new matter to its apex, not by any general distension of its vessels or fibres; which new matter appears, from the experiments of Bonnet and Du Hamel, to descend from the cotyledons. The first motion, therefore, of the fluids is downwards, towards the point of the root; and the vessels which carry those fluids are similar to those which are subsequently found in the bark. In support of this opinion, he mentions some observations he has made on the progressive changes which take place in the radicle of the horse-chestnut. From these it appears, that when the roots were considerably elongated, and not till then, alburnous tubes were formed, and that as soon as these tubes had acquired a sufficient degree of firmness, they appeared to begin their office of carrying up the aqueous sap; at which time, and not sooner, the leaves of the plumula expanded. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon, being fed by the alburnous tubes and central vessels; and the true sap is discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark. Here one part of it produces the new layer of wood (or new epidermis when that is to be formed), and the remaining part enters the pores of the wood already formed, and mixes with the ascending aqueous sap.

The author thinks it probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap, as in the sycamore; it was found to become more sensibly sweet in its progress in the root, in the spring, although he could never detect

the slightest degree of sweetness in decoctions of the wood in winter. He therefore is inclined to believe, that the saccharine matter is generated by a process similar to that of the germination of seeds; and that the said process is always going on during the spring and summer; but that towards the conclusion of the summer, the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter. He says also, that he has some reasons for thinking that the true sap descends through the alburnum, as well as through the bark; and that he has been informed, that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability.

Mr. Knight concludes by observing, that he conceives himself to be in possession of facts, which prove that both buds and roots originate from the alburnous substance of plants, and not, as he believes is generally supposed, from the bark.

*On the Action of Platina and Mercury upon each other.* By Richard Chenevix, Esq. F.R.S. M.R.I.A. &c. Read January 10, 1805. [*Phil. Trans.* 1805, p. 104.]

Mr. Chenevix, in the month of May 1803, presented to the Royal Society a paper, which was printed in the Philosophical Transactions for that year, respecting the nature of a metallic substance which had been offered to the public as a new simple metal, under the name of Palladium. In that paper he not only attempted to prove that the said substance, instead of being a simple metal, was merely a compound of platina and mercury, but he also described certain processes by which he had been enabled to produce it. He now expresses his mortification to learn that the processes he there recommended, as the least likely to fail, have been generally unsuccessful; and confesses he has reason to believe "that the nature of palladium is considered by most chemists as unascertained, and that the fixation of mercury by platina is by many regarded as visionary."

In France, he says, the compound nature of palladium has been more generally credited; M. Guyton, who was appointed by the National Institute to make a report upon Mr. Chenevix's experiments, having repeated some of them, and having been led by the results to the same general conclusions as Mr. Chenevix.

Messrs. Fourcroy and Vauquelin also made some experiments upon the subject; but as about this time a new metal had been discovered in crude platina by Mons. Descotils, the above-mentioned chemists were led to suppose it probable that the new metal was concerned in the production of palladium; and finally declared, as their opinion, that the substance called palladium does not contain mercury, but is formed of platina and the new metal of M. Descotils. Mr. Chenevix adduces several arguments to show that this opinion is not well founded; and in the latter part of his paper, he says, that